RESTRICTED ~~CONFIDENTIAL~~ ~~SECRET~~

AIR MINISTRY.

M.R.P.106.
20th May, 1943.

METEOROLOGICAL OFFICE
23 MAY 1944
LIBRARY

METEOROLOGICAL RESEARCH COMMITTEE.

Remarks by Sir G. Walker upon Dr. Brooks' proposed method of long range forecasting by means of pressure wave analysis.

1. My impressions before I went to Dunstable to get data and see Brooks' methods in use were:-

(a) The difference periodogram is only approximate. There is no measure of its probable error and the degree of certainty of its results is unknown.

(b) Since symmetry points depend on oscillations for their existence, the fact that they break down shows that the waves on which they depend are unreliable.

(c) The workers, mostly German, who have used waves are not in agreement. Each believes in his own set of four or five, but the sets are not identical. The existence of some of the waves must be doubtful.

(d) The waves of 72, 48 and 36 days would produce rises and falls in the pressure charts used in deriving the "trends", and the latter are scarcely independent of the waves. Further, assuming independence, if the correlation between the actual pressure and that foretold by waves, were .25, and between the "trend" forecast and the actual were also .25, the joint correlation would be $(.25 \times \sqrt{2})$ or .35, which is not good enough - mere agreement between the two indications does not create certainty; when the trend is incorrect agreement means error.

(e) The result of the examination of 20 years' pressure (Fig.3) seemed too discontinuous to be valid. Periodograms, either correlation or harmonic, in such cases show maxima or minima in smooth curves, but not high ordinates next to zeros.

(f) Stumpff and others have been hard at work upon waves since 1930 on lines essentially the same as Brooks, but no reliable method of forecasting has come from them in 12 years (Bull, Amer.Met.Soc., 23, 107-8, 1942).

2. I am glad to say that these difficulties were considerable lightened as the result of fuller information and discussion. Taking them in turn:

(a) The difference periodogram is admittedly approximate. In unreliable hands it might prove misleading. But Brooks and his assistant Miss Carruthers impressed me as scrupulously careful, and its reduction in labour has enabled them to discuss more material than if they had followed more orthodox methods.

(b) When the amplitudes and phases of a wave are plotted over a wide area it is found that the place of maximum amplitude may for some time have been near station A, but data of the next weeks show it at B, and after this it may be at C. Its disappearance from A would

FGM

P 9020

/probably

probably spoil a symmetry point there; but if the motion can be recognised and allowed for in forecasting there is no necessary failure. The correlation between successive values of the amplitudes at the same place, taking phase into account, averages only about 0.1; but if motion could be allowed for, a material improvement would be effected.

(c) Believers in waves do not in general analyse at one process series of more than about 72 days; if a series of 180 or 360 days is analysed the amplitudes found must be smaller because the waves only occur for part of the time and successive appearances may be in different phases. But this limitation means that the resolving power is small, and uncertainties of a day or two in the time of an oscillation are inevitable. Hence Stumpff's 20 day waves and Brooks' of 18 days may have much in common.

(d) The discontinuity is mainly due to the method of attack. The time intervals analysed are relatively short, and a few waves of large amplitude will be registered while smaller ones, though occurring more continuously, will not. In analyses of 120 days or more the successive amplitudes will form a continuous curve.

3. Brooks has been good enough to let me have data for use in checking some of his results; but I do not expect any outstanding consequences. Without delay therefore I may state my present impressions:

(a) The changes of the waves in place and time have not been cleared up. It may be that they depend on outbreaks of ice or some such phenomenon, but there is no certainty as yet of any explanation.

(b) Investigations by ordinary statistical methods show relationships that are real - for example the correlation of 0.6 between the N.Atlantic oscillation (essentially that accepted as valid by Hildebrandsson, Petersen, Hann and others) from December to February and the outflow of ice into the N.Atlantic from March to July, based on 56 years' data (B.A. Report, p.41, 1933). Also I have published coefficients up to .5 for forecasting temperatures - mainly in northern Europe. But I don't imagine that a coefficient of less than .65 is of serious use and doubt if it is forthcoming. Further, if there were adequate ground for expecting on 31st May, that the mean pressure for June would have a certain pattern, would that be of appreciable value in enabling us to forecast the weather, say of 7th June, in a manner adapted for military purposes?

(c) I am glad that Brooks is to have a period for putting his ideas to the test.